

[Reprinted from THE JOURNAL OF NERVOUS AND MENTAL DISEASE, Vol. 77,
No. 2, February, 1933.]

BIOGRAPHY

SCIENCE IN THE CLINIC AS EXEMPLIFIED BY THE LIFE AND WORK OF JOSEPH BABINSKI

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I take it that one of the purposes of the society is to recognize and promote a closer relation between the science and the art of medicine. In most American medical schools no man can really attain distinction without a clear conception of what scientific evidence means, but it is unfortunately also true that a high academic standing generally gives little or no indication of his ability to practise medicine. It is therefore very reasonable to enquire how far science and the healing art can really be combined in the same individual. Osler is sometimes cited, I think unjustly, as the supreme instance of a man who had mastered the art without the science. Is it essential that these two capacities remain separated? My reply is emphatically "No" and I should like this evening to tell you why I think so.

I

Within the past few years the problem of the relation of the laboratory to the clinic has presented itself in more direct fashion than ever before. Those who are able to devote themselves purely to medical research are not likely to increase in numbers in the near future, and if the present economic situation continues, the ranks of biological investigation will diminish sharply. I believe that the medical schools of this country are training far too many men who

¹ Annual Alpha Omega Alpha Lecture, College of Physicians and Surgeons, New York, December 13, 1932.

have only scientific investigation in view, and that unless our present social organization is radically changed, their future must be regarded as insecure. But for those who have both medical and scientific biological training, the situation is quite different—the Committee on the Costs of Medical Care notwithstanding! Imagine for a moment what would happen if *all* the research workers in the field of laboratory medicine were cut off and had to walk the streets in search of work. With a few exceptions they would be a very sorry sight, but at present no sorrier, if viewed from a distance, than the remaining body of practising physicians who would find that they had no serum, no standardized vaccine and few new remedies; and, unless some extraordinary change occurred, they would find, as the years advanced, that medicine was progressing but little.

Among those who have devoted themselves to active practice there have been highly significant exceptions to the generalization that few major advances in scientific medicine have come from practitioners; and when the economic wolf is at the door of our social organization I think it will pay to examine these exceptions with some care, for it is my firm conviction that fundamental advances of medicine of the future will come not alone from the cloistered laboratory monk, but from men in the front line trenches who, seeing patients daily, are the first to be brought in contact with the major problems of disease. They will bring this about, however, only if they carry to their daily task sound training in science combined with that human insight without which the practice of medicine becomes a dull and uninspired routine.

My beliefs concerning the future of medicine are based on two surmises. One, being an economic prognostication, is probably unsound. It is simply this that the number of laboratory workers which the present social system will support is strictly limited, and for obvious reasons it is likely to diminish, however much we may educate the philanthropic public. The other surmise requires a word of explanation. Many physiologists have devoted their attention in the past to the study of the function in rats, cats and dogs and they have been far too uncritical in generalizing from results obtained in these lower forms to the functions of the human animal. To be sure, progress has been made, the functions of the vagus nerve, insulin, the great experiments on dietary deficiencies and a hundred other things come to mind, but there have been errors, which would have not occurred had similarly well controlled observations been carried out in human beings. I was much interested some years ago in preparing a volume of *Readings in the History of Physiology* (1)



to discover on analyzing the material that fourteen of the seventy-odd significant advances which had been singled out for inclusion in this volume had been based upon the human experiment—*e.g.*, the studies of Spallanzani, Beaumont, Haldane, Thomas Lewis. This ratio seems to me significantly high in view of the fact that the majority of investigators in the past have automatically excluded human beings as objects for experiment. However, it has become more and more clear that we shall never know what the human animal does in a given situation, or how its various organs function, unless studies are ultimately carried out on human beings. This would seem to be a truism and yet it is too little appreciated and I look upon it as perhaps the most important fact that a medical student can learn. For this and other reasons I firmly believe that much of the progress of physiology and medicine in the future will come from men who are handling human beings—intelligently.

II

If one looks about with a view to seeing who among contemporary physicians have advanced the science of medicine by the study of human material the numbers are not great, but they are highly significant. In this country one thinks first perhaps of Minot and Murphy's (2) work on pernicious anemia, for the discovery of the liver diet could never have been made on a laboratory animal. One thinks also of Cushing's studies on the postcentral convolution of conscious human beings (3); and in quite a different sphere of Landis' (4) beautiful analysis of the human capillary pressure at the base of the nail. In England the first name that comes to mind is that of Thomas Lewis (5) whose analysis of the triple response in human skin has proved basic to all theories of the reaction of tissues to injury; and in Germany to Otfried Foerster whose observations on the cortex (6) and upon the sensory pathways in the human spinal cord (7) have been revealing and unique, and have given a body of information which could never have been obtained from animal experimentation. These instances might be multiplied but I think that my point can be illustrated more effectively by considering the career of a master of clinical neurology—one of the greatest of all time—Joseph Babinski, who died about six weeks ago at his home in Paris.

III

Babinski's name is familiar to every student of medicine through the sign of the upgoing toe which bears his eponym, and his career stands as a great inspiration to anyone who proposes to combine

science and clinical medicine, because here we find a man who restricted himself to the clinical field and in so doing achieved his fame. His life and work illustrates poignantly that research need not be confined to the laboratory and that some of the greatest achievements in medicine may come from penetrating clinical observation alone.

Born of Polish parentage on November 17, 1857, Babinski received all of his education in France and apart from one or two short holidays spent his entire life in that country. He took his medical degree from the University of Paris in 1885 and submitted for his thesis an important study upon the pathology of multiple sclerosis in which he attempted to correlate the pathological findings of the disease with clinical signs. His training as a student thus came entirely from French sources. It would be difficult, however, to understand the intellectual atmosphere in which he found himself without some knowledge of the history of neurology and physiology, particularly that of France, before his time.

It is not difficult to trace the history of neurophysiology prior to 1885 for fundamental discoveries were few and can be enumerated almost on the fingers of two hands. Galen (8), the private physician of Marcus Aurelius, had discovered the functions of recurrent laryngeal nerve and made pioneer observations on the results of transection of the spinal cord and of lateral semi-section of the cord at various levels. He pointed out that upper cervical transection caused immediate death from paralysis of respiration; but animals survived with transection below the level of the fifth cervical. He described in no uncertain terms the differences between abdominal and intercostal respiration, but the distinction between sensory and motor nerves was not clearly made and there is no suggestion in the work of Galen of the separate function of anterior and posterior nerve roots. He put forward the doctrine of "animal spirits." For him a nerve was merely a tube which communicated a subtle essence, fluid in character, from the central nervous system to the muscles. The muscles on receiving this fluid swelled up and shortened. This simple explanation of muscular contraction under the influence of the nervous system survived almost without question till the end of the eighteenth century.

After Galen little of significance was added to the knowledge of the functions of the nervous system until the theory of reflex action had been promulgated. To be sure Leonardo had studied the cerebral ventricles and Vesalius had repeated many of the Galenic experiments on the spinal cord and recurrent laryngeal nerve, but they added

little that was new to the knowledge of function. With Descartes in the middle of the seventeenth century, however, we find the first glimmering of reflex action (9). The figure of the child putting its finger in the fire and the sensation of heat being transmitted to the pineal gland where it is reflected back to the muscles of the forearm is undoubtedly familiar. He also touched on the importance of the question of reciprocal innervation of antagonistic muscles, especially those of the eye.

But to the far-seeing clinical neurologist and professor of physiology at Edinburgh, Robert Whytt, we owe the first description of reflex action. A man of large clinical experience, Whytt had studied in his patients the response of the iris to light, and he had observed other involuntary reactions of man and animals. In the course of his practice he came upon the case of a boy who had several times lapsed into coma from an intracranial condition of unknown character. He observed during the last weeks of the child's life that his pupils failed to respond to light. There must, he argued, be something blocking the channels by which the stimulus of light was conveyed to the brain or in the path by which the brain conveyed its influence to the muscles of the iris. He obtained an autopsy on the case and found that the child had a large cyst pressing the midbrain in the region of the optic thalamus. Here, therefore, is a further instance of a man with scientific instincts who studied clinical material, and as a result of his penetrating analysis gave the first adequate description of a reflex reaction so paving the way for all the great subsequent development in the knowledge of the nervous system. Not only did he describe the reflex, but he gave a fairly accurate localization of the central pathway involved. Whytt's book (10), *An Essay on the Vital and Other Involuntary Motions of Animals*, was published in 1751, but its significance was not fully appreciated until the discovery of the sensory function of the posterior roots. Whytt also wrote other books on nervous diseases in one of which he gave the first description of tuberculous meningitis.

The circumstances attending the discovery of the functions of the anterior and posterior roots were unusual, and culminated in an acrimonious controversy between Charles Bell and the French physiologist Magendie. I have never been able to satisfy myself that Charles Bell's claim to priority was really justified, and I believe the chief laurel should go to Magendie because his demonstration was more complete, and perhaps one should add, his subsequent behavior was more that of a gentleman. The discovery was formally announced in 1822.

At the same time in France a young man bearing the name of Marie Jean-Pierre Flourens began to attract the attention of all those interested in the functions of the nervous system, and he deserves to be remembered more particularly for his analysis of the functions of the cerebellum first published in 1824 (11). Others had worked upon the cerebellum before his time, but no one had really described the fundamental disturbance of locomotion which follows cerebellar injury and no one had previously appreciated that destruction of this part of the brain leaves the intellectual faculties unimpaired. Flourens attempted to remove the cerebellum in twenty-one different species of animals and found in each that the operation caused incoordination of movement, staggering gait and slowness in initiation of movements. The description which he gave of the cerebellar gait leaves little to be desired. As far as is known, however, Flourens did not study clinical cases, and he made little attempt to apply the results of his investigation to man except for one shrewd deduction, namely, that the cerebellar gait in animals was similar to the corresponding gait in a human being under the influence of alcohol. May it not be, he asked, that alcohol acts primarily upon the cerebellar centers? As far as I am aware no one since has adequately analyzed or followed up this brilliant suggestion.

After the work of Flourens there is little to record in the history of the physiology of the nervous system until the time of Goltz and his celebrated experiments upon decorticated dogs. In this interval, to be sure, Marshall Hall (12) described and studied the spinal animal, the Webers (13) discovered the action of the vagus nerve, and Brown-Séquard (14) the syndrome of semi-section of the spinal cord which appropriately bears his name. These studies were important but they did not give the impetus that came from Goltz (15). Goltz had removed the hemispheres on both sides of a series of dogs and had shown that the animals may live indefinitely since all the vegetative functions continued to be carried on without serious impairment. Decorticated animals, to be sure, seemed "stupid," *i.e.*, they are unable to respond appropriately to simple changes in their external environment. There is a monotonous sameness in all their movements and behavior, signs of rage are exhibited in response to almost any stimulus, touch, light, sound, food or even stroking. Goltz, however, fell into the error of denying the idea of localization of function and he was unwilling even to admit the existence of the motor area.

The existence of a specific region of the cerebral hemispheres responsive to electrical excitation had become established in the early

seventies by Fritsch and Hitzig (16, 17). They found that the precentral convolutions of monkeys and the sigmoid convolution of dogs caused movement of the extremities on the opposite side when stimulated. To David Ferrier (18, 19) who died only a few years ago must be given the credit of the first systematic study of the results of lesions of the excitable area. He found in monkeys that if he destroyed the part of the precentral convolution giving responses of the hand, the extremity became temporarily paralyzed. At the International Medical Congress of 1881 he demonstrated such an animal, and Charcot, Babinski's master, on beholding the hemiplegic monkey, exclaimed with his characteristic French enthusiasm, "C'est un malade." It is difficult for us now to visualize the fervour with which these early experiments were greeted. It was obvious, among other things, that the common clinical conditions of hemiplegia could be explained on the basis of a lesion involving the fiber systems arising in the cerebral cortex.

IV

Into this atmosphere of ferment and enthusiasm Babinski found himself plunged as a student. Many of the most fundamental conceptions in neurology had yet to be established. The knee-jerk had only just been described (20, 21) and physiologists (22) were unwilling to look upon it as a true reflex because its latency was so brief. The plantar reflexes had not been studied, the abdominal reflex was not used and other tendon reflexes were only just being recognized. One might say parenthetically that it seems almost incredible that the knee-jerk which is so obvious a phenomenon should not have been described until the memorable papers of Erb and Westphal in 1875 (20, 21).

Of Babinski's early career we know very little (23). His father was an engineer and a man of great learning and his mother a widely read, cultivated woman who saw to it that her two sons, Joseph and Henri, were brought up in accordance with the highest tradition of continental education. The two boys had their preliminary education in the Polish school known as Batignolles. Henri ultimately became an authority on mining engineering.

I think it significant that Babinski laid a broad foundation in general medicine before he undertook to specialize. How much better it would be if our modern "specialists" would see the wisdom of this example. As a student in 1879 he served under the internist Cornil. He was much influenced by Bucquoy of whom he frequently spoke in later life, and he was also on intimate terms with Charrin,

Darrier, Guignard, Suchard and Vaquez. From Vulpian, the celebrated clinical neurologist, he derived his first interest in the nervous system, but it was the fortunate appointment as chief of clinic to Charcot that really determined Babinski's subsequent career. He remained with Charcot for five years and in 1890 he was asked to take charge of the ancient hospital de la Pitié. Here he built up a large neurological clinic along the lines of that of Charcot at Salpêtrière; he remained at la Pitié until he retired five years ago.

It has been said of Babinski that he preferred studying the living to the dead, and this is, I think, the secret of his success as a clinician. Though interested in pathology, the study of morbid changes was for him a means, and not an end, in clinical medicine. He wasted no time in reporting chance findings of the autopsy rooms, but a chance finding in a living patient was followed and analyzed with unswerving singleness of purpose with autopsy or operation as the ultimate goal.

His clinical contributions cover the whole gamut of medicine and neurology. His earlier publications (24) dealt with scattered problems of general medicine, hydatid cysts, malignancy, typhoid, etc. After 1885 one finds an increasing number of neurological papers and after 1890 he confined himself almost entirely to clinical neurology. From 1883 until 1930 he published on an average of from three to four papers a year, and in some years there were many more than this.

I shall mention first his work on plantar reflexes for it is this that has caused his name to be frequently on the lips of every student of medicine in every corner of the globe. In a preliminary note published in the *Société de Biologie* in 1896 he stated (25) that "in certain cases of paralysis the toes on the affected side, instead of flexing when the sole is stimulated, execute an extensor movement on the metatarsal." This was the first mention of the phenomenon of the upgoing toe, though Vulpian, Babinski's first teacher, is said to have observed it without being aware of its significance. Two years later Babinski (26) published a detailed account of his studies on the pathological plantar reflex, and summarized the evidence for regarding it as a sign pathognomonic of pyramidal tract injury. The phase of fanning of the toes sometimes seen as a part of the pathological plantar reflex was not described until 1903 (27) and at this time Babinski was careful to point out that this phase of the response was not invariably observed and that there was no absolute proof of its being specifically associated with pyramidal tract disease. The significance of these abnormal plantar reflexes were quickly recognized and they were brought to the attention of English neurologists by James Collier's paper in *Brain* (28) in 1899. Immediately after

this a large literature quickly developed. In recent years I myself have been very deeply interested in the phenomenon since it has proved of great theoretical interest in connection with physiological studies on the nervous system of various primate forms, such as the monkey, gibbon, baboon and chimpanzee (29). In the lower monkeys nothing corresponding to a Babinski response could be detected after destruction of the pyramidal tract, whereas in the higher apes, such as the chimpanzee, isolated destruction of the foot area of the motor cortex gives rise to a marked pathological reflex similar in all respects to that seen in the human being. We believe, therefore, that the reflex serves as a useful index to the degree of cortical dominance present in any given form and that by studying it comparatively we can gain some insight into the evolutionary background of the human brain.

Babinski's contribution to the study of cerebellar function was scarcely less fundamental. Few people are familiar with the fact that he is responsible for introducing the terms now universally employed in describing the symptoms of cerebellar disease. In a paper published in 1899 (30) he pointed out that the fundamental disability is a "*perturbation de la faculté d'association des mouvements, la synergie musculaire; cette perturbation peut, en raison de son origine, être désignée sous la dénomination d'asynergie cérébelleuse.*" That is, in cerebellar disease the various muscle groups which normally contract and relax in appropriate temporal sequence giving smoothly coördinated movement (synergy) contract in haphazard fashion, and temporal sequence is grossly disturbed (asynergia). This was a very penetrating observation and the appropriateness of the terms used to describe this "perturbation" of movement have caused them to survive.

In later communications he gave a number of very simple clinical methods for establishing the presence of asynergia; thus in cerebellar disease patients are unable to perform alternating movements requiring rapid repetition of action of the same muscle groups, *i.e.*, they cannot readily supinate and pronate their forearms or tap their fingers against the thumb in rapid succession. This particular sign of asynergia was designated *adiadocokinesia* (31), a term derived from two Greek words meaning 'repeated' and 'movement.' He pointed out furthermore that there was difficulty in initiation of movement and that at times the patient tended to retain a given posture or pattern of movement for abnormally long periods. The similarity between this sign of asynergia and true catalepsy was striking and it was therefore designated *catalepsie cérébelleuse* (32).

Finally he pointed out that voluntary movements cannot be accurately calculated in cerebellar deficiency because of a tendency to overshoot the mark and to this sign he gave the appropriate and now familiar name *hypermetria* (33).

It would be quite impossible to enumerate all of Babinski's other contributions to the broad field of neurology. He was one of the first to point out that the loss of the knee-jerk and other prominent symptoms of tabes were due to interruption of the posterior roots (34) and with Charpentier he gave a pioneer description of the abolition of the pupillary response to light in cases of cerebral syphilis (35). A year before Fröhlich described the adiposo-genital syndrome in the male, Babinski published a case report (36) entitled: "A tumor of the pituitary body without acromegaly and with arrested development of the genital organs." He should thus be given priority in the description of this important syndrome in so far as it relates to the female.

I had not realized until a few weeks ago that Babinski also did much to promote neurological surgery in France and with Lecène and Bourlot (37) successfully removed the first spinal cord tumor to be operated on in France. Babinski also urged cerebral explorations (38) in cases of brain tumor, and his pupils de Martel and Clovis Vincent have contributed notably to the advancement of cerebral surgery in France.

Babinski's studies in the field of psychiatry (40) also merit close attention. His master, Charcot, never wearied of studying the manifestations of hysteria and many of his clinics were made dramatic by his spirited demonstrations of hysterical patients. Babinski at an early date came to disagree with Charcot's interpretation of the major symptoms of hysteria and he subsequently applied himself to the study of this condition with energy and a critical attitude that commends itself to all thoughtful clinical investigators. Perhaps his most important contribution lay in establishing satisfactory criteria for differentiating hysterical manifestations from those caused by organic lesions of the nervous system. He pointed out that deep and superficial reflexes remained normal in hysterical patients—*i.e.*, that the sign of the upgoing toe is never present—and he believed that all hysterical symptoms are capable of being reproduced and in the first instance are actually produced by suggestion. By the same token they may be made to disappear by verbal persuasion. It was natural that Babinski should have spent a large part of his time in heated controversy concerning his interpretation of hysteria. The idea of dissociation and the Freudian conception had little appeal to his

matter-of-fact mind and he was never weary of defending his conservative position. Whether or not we disagree with his rather reactionary interpretation, the fact remains that, during the Great War, Babinski was probably more successful in dealing with the war neuroses than any practitioner in France, or perhaps in any other country. He used strong-arm measures and he had uncanny success in distinguishing those who had true neuroses from those who were malingerers. An English translation of his book on hysteria was published immediately after the war (41).

V

Babinski's career seems to me to have this message to offer. He gave himself the broadest possible training in general medicine before he came to specialize. He chose as his mentors the best clinical teachers then to be found in France and he remained loyal to them until his death. His scientific achievements were based entirely upon the study of clinical material. Painstaking observation and study, coupled with an alertness of mind that allowed nothing to escape him caused Babinski to see far more deeply into the mysteries of the nervous system than most men who have had the advantage of the experimental approach to the subject. And it is a curious fact that in most of his papers Babinski was silent about relevant work from physiological laboratories. There is, of course, ample evidence that he was familiar with the experimental literature, but he clearly did not regard laboratory experimentation as essential for a career as a scientific clinician. Had he been an experimentalist he might have gone even further, but those who wish to emulate him in the field of medicine will do well if they achieve as much as he, and for them Babinski leaves the memory of a great name and the inspiration of a great example.

BIBLIOGRAPHY

1. Fulton, J. F. *Selected Readings in the History of Physiology*. Springfield, Ill.: Charles C. Thomas, 1930, xx+317 pp.
2. Minot, G. R., and Murphy, W. P. "Treatment of Pernicious Anemia by a Special Diet." *J. Am. Med. Assn.*, 1926, **87**, 470-476.
3. Cushing, Harvey. "A Note upon the Faradic Stimulation of the Postural Gyrus in Conscious Patients." *Brain*, 1909, **125**, 1-5.
4. Landis, E. M. "Micro-Injection Studies of Capillary Blood Pressure in Human Skin." *Heart*, 1930, **15**, 209-228.
5. Lewis, Thomas. *The Blood Vessels of the Human Skin and Their Responses*. London: Shaw and Sons, 1927, xv+332 pp.
6. Foerster, Otfried. *Die Leitungsbahnen des Schmerzgefühls und die chirurgische Behandlung der Schmerzzustände*. Berlin: Urban & Schwarzenberg, 1927, 360 pp.
7. ——— "The Cerebral Cortex in Man." *Lancet*, 1931, **2**, 309-312.
8. Prendergast, J. "Galen's View of the Vascular System in Relation to that of Harvey." *Proc. Roy. Soc. Med. (Sect. Hist. Med.)*, 1928, **21**, 1839-1848.

9. Descartes, R. *De homine figuris et latinitate donatus a Florentis Schuyt.* Leyden, Leffen and Franciscus Moyardum. [34]+121+[8]+85 pp.+1, 1, 6 plates, 1662.
10. Whytt, R. *An Essay on the Vital and Other Involuntary Motions of Animals.* Hamilton, Balfour and Neill, 1751, vii+392 pp.
11. Flourens, M. J. P. *Recherche expérimentales sur les propriétés et les fonctions du système nerveux, dans les animaux vertébrés.* Paris, Crevot, 1824, xxvi+331 pp.
12. Hall, Marshall. "On the Reflex Function of the Medulla Oblongata and Medulla Spinalis." *Philos. Trans.*, 1833, 635-665.
13. Weber, E. F., and Weber, E. H. "Experimenta, quibus probatur nervos vagos rotatione machinae galvano-magneticae irritatos, mortem cordis retardare et adeo intercipere." *Omodei's. Ann. Univ. Med.*, 1845, 116, 227-228.
14. Brown-Séquard, C. E. *Course of Lectures on the Physiology and Pathology of the Central Nervous System.* Philadelphia: Collins, 1860, xii+276 pp.
15. Goltz, F. "Der Hund ohne Grosshirn." *Pflüger's Arch.*, 1892, 51, 570-614. See also discussion on the localization of function in the cortex cerebri. *Trans. Internat. Med. Congress*, 1881, Vol. 1, pp. 218-241.
16. Fritsch, G. F., and Hitzig, E. *Ueber die elektrische Erregbarkeit des Grosshirns.* *Arch. Anat. Phys. wissensch. Med.*, 1870, 37, 300-332.
17. Hitzig, E. *Untersuchungen über das Gehirn.* Berlin: A. Hirschwald, 1874, xiii+276 pp.
18. Ferrier, D. "Experimental Researches in Cerebral Physiology and Pathology." *West Riding Lunatic Asylum Reports*, 1872, 3.
19. Ferrier, D. *The Functions of the Brain.* London: Smith, Elder and Co., 1876, xvi+323 pp.
20. Erb, W. "Ueber Sehnenreflexe bei Gesunden und bei Rückenmarkskranken." *Arch. Psych. Nervenhe.*, 1875, 5, 792-802.
21. Westphal, C. "Ueber einige Bewegungs-Erscheinungen an gelähmten Gliedern. II. Ueber einige durch mechanische Einwirkung auf Sehnen an Muskeln hervorgebrachte Bewegungs-Erscheinungen." *Arch. Psychiat. Nervenhe.*, 1875, 5, 803-834.
22. Waller, A. D. "On the Physiological Mechanism of the Phenomenon Termed 'Tendon Reflex'." *J. Physiol.*, 1890, 11, 384-395.
23. The details of Babinski's life have been described at some length by Guillaumin, G., "J. Babinski (1857-1932)." *La Presse Médicale*, Nov. 12, 1932, p. 2, and Charpentier, A., "Babinski," *Figaro*, 30 Octobre, 1932, p. 2, and Vincent, C., *Rev. Neurol.*, 1932, 2, 441-446. See also obituaries in *Arch. Neurol. Psych.*, Jan., 1933; *N. Eng. J. Med.*, 1932, 27, 898-899, and notices in *Brit. Med. J.*, 1932, 2, 892 and 988; *J. Am. Med. Assn.*, 1932, 99, 2045, and *Lancet*, 1932, 2, 1122; Vaquez, H., *Nécrologie, Joseph Babinski (1857-1932)*, *Bull. Acad. Méd.*, 1932, 108, 1264-1273. There is also an appreciation by M. W. Thewlis, *N. Y. Med. J.*, 1922, 115, 295.
24. Babinski, J. "Exposé des travaux scientifiques du Dr. J. Babinski." Paris: Masson & Cie, 1913, 234 pp. This unfortunately only includes Babinski's French titles. Many of the same papers were published in Polish journals, *e.g.*, No. 33, which appeared in *Gaz. lek. Warszawa*, 1909, 29, 893, 958, 1000, 1020, 1049; there were also many translations of his papers into English, *e.g.*, see *Interstate Med. J.*, 1905, 12, 167-177; 1909, 16, 171-190.
25. ——— "Sur le réflexe cutané plantaire dans certaines affections organiques du système nerveux central." *Compt. rend. Soc. biol.*, 1896, 3, 207-208.
26. ——— "Du phénomène des orteils et de sa valeur séméiologique." *Semaine méd.*, 1898, 18, 321-322.
27. ——— "De l'abduction des orteils." *Rev. neurol.*, 1903, 11, 728-729, 1205-1206.
28. Collier, J. "An Investigation upon the Plantar Reflex, with Reference to the Significance of Its Variations Under Pathological Conditions Including an Enquiry into the Aetiology of Acquired Pes Cavus." *Brain*, 1899, 22, 71-99.

29. Fulton, J. F., and Keller, A. D. *The Sign of Babinski. A Study of Cortical Dominance in Primates.* Springfield, Ill.: Charles C. Thomas, 1932. xi+165 pp.
30. Babinski, J. "De l'asynergie cérébelleuse." *Rev. Neurol.*, 1899, 7, 784-5, 806-816.
31. ——— "Sur le rôle du cervelet dans les actes volitionnels nécessitant une succession rapide de mouvements. (Diadococinésie.)" *Rev. Neurol.*, 1902, 10, 1013-1015.
32. ——— "De l'équilibre volitionnel statique et de l'équilibre volitionnel cinétique. Dissociation de ces deux modes de l'équilibre volitionnel. Asynergie et catalepsie." *Rev. Neurol.*, 1902, 10, 470-474.
33. ——— "Quelques documents relatifs à l'histoire des fonctions de l'appareil cérébelleux et de leurs perturbations." *Rev. mens. Méd. int. Thérap.*, 1909, 1, 114-129.
34. ——— "Sur le réflexe tendon d'Achille dans le tabes." *Soc. Méd. Hôp. Paris*, 1898, 15, 679-682.
35. ——— "De l'abolition des réflexes pupillaires dans ses relations avec la syphilis" (en collaboration avec Charpentier). *Soc. Méd. Hôp. Paris*, 1901, 17, 502.
36. ——— "Tumeur du corps pituitaire sans acromégalie et avec arrêt de développement des organes génitaux." *Rev. Neurol.*, 1900, 8, 531-533.
37. ——— Lecène and Bourlot. "Tumeur méningé. Paraplégie crurale par compression de la moelle. Extraction de la tumeur." Guérison. *Rev. Neurol.*, 1911, 19, 653. [In title.]
38. ——— "Stase papillaire guérie par la trépanation craniennne." *Rev. Neurol.*, 1901, 9, 266-267. See also "Apropos de la communication de M. Ballet sur trois cas de gliomatose cérébrale." *Ibid.*, 155.
39. ——— "Utilité de la crâniectomie décompressive dans les tumeurs cérébrales" Leçon daite à la Pitié. *J. Méd. Chirurg. prat.*, 1910, 81, 407-411. See also "Stase papillaire bilatéral. Cécité presque complète. Crâniectomie décompressive avec incision de la dure-mère. Guérison" (en collaboration avec Chaillous et de Martel). *Rev. Neural.*, 1912, 20, 638-640.
40. ——— "Contractures organiques et hystérique." *Soc. méd. Hôp. Paris*, 1893, 10, 327-343. See also "Émotion et hystérie" (en collaboration avec Jean Dagnan-Bouveret), *J. Psychol. norm. path.*, 1912, 9, 97-146.
41. Babinski, J., and Froment, J. *Hysteria or Pithiatism and Reflex Nervous Disorders in the Neurology of War.* London: University of London Press, 1918, xxii+311 pp.

